must have responded in the ways they do, or have responded at all to the environment, I meant only that the theory ought to fulfil the conditions which other physical theories are bound to satisfy, *i.e.* to account for the principal facts of the case. I had no reference to any subsidiary hypothesis which might help the matter. Dr. Romanes rightly says that it lies not with the evolutionist to show that variations may not have been intellectually planned or guided. But when he assigns the whole results to known physical causes and discards the factor of intelligence, he is bound to render their adequacy at the least conceivable.

It may now be seen, I trust (and the context might have made it clear), that, in asking Dr. Romanes if he was quite sure that any other cause than intelligence could adapt organisms to their environment gradually, I was not inviting him to guesses "about the possibilities of supernatural creation," but to a reconsideration of his antithesis between special (and as he will have it, sudden) creation, requiring intelligence, and gradual evolution, which might dispense with it; and I was intimating that he had not shown how the latter could dispense with it. The problem was: Given plants and animals with certain structures and certain adaptations to their environment, to be changed into other forms with other structures equally well adapted to a more or less changed environment, how to do this solely by the action of said environment. Answer: By the killing out of all which have not somehow or other acquired the particular structure and adaptation they needed.

But now comes an important qualification: "The evolutionist may freely admit that natural selection has probably not been the only physical cause at work, and even that the variations supplied to natural selection may not have been wholly fortuitous, but may have occurred along favourable lines as responses of the organisms to their physical surroundings"; and Dr. Romanes calls my attention to a statement of his that it may be so in an essay which I regret that I have not read. He continues, however: "But such admissions would make no change in the logical aspect of the case; for, however many supplementary causes of this kind we may choose to imagine as possible, the evolutionist is bound to regard them as all alike in this: that they are of a physical or natural kind."

"Physical or natural kind." The agency which explained away all implication of design was in the strict sense physical, being the action of the environment on the organisms. It is now extended to whatever is *natural*, that is, to whatever occurs in the course of nature, presumably under established laws; and it is assumed that whatever so occurs is thereby void of all *evidence* of intellectual intention (we need not regard the difference—if any there be in such relations—between general and special design, the question being wholly one about the grounds of any *evidence* of design in nature). To me it is wholly probable that existing species and their special adaptations became what they are in the course of nature. And my argument is that, if "such a purely physical cause as natural selection" leaves these adaptations still unaccounted for, whatever implication of designed origination there formerly was still holds, and may hold, although the series of natural causes be practically endless.

Then as to such causes being all of a piece, so that pure physics may explain all biology. Doubtless in a certain sense all nature is of a piece. But in another sense—the very one we are concerned with—it is of at least two pieces; no matter how it came to be so. One of them is pervaded by an element of its own—that of *direction of action to ends*—which is more and more manifested as we rise in the scale of being, but is characteristic of all organisms. That seems to lay a foundation for a difference in the quality of the "inference which can be drawn by the human mind [*quoad* design] from the province of natural science." This difference might have made Dr. Romanes hesitate to draw, from scientific premisses, the downright conclusion that "the facts of organic nature present no evidence of design of a quality other or better than any of the facts of inorganic nature."

Here lies our whole contention. We agree that natural science leaves aside the question whether evolution and design in nature are compatible or not, this being only a phase of the enigma which was as puzzling before evolution was dominant as it is now. We suppose, too, that the difficulty of conceiving how design can coexist with the natural evolution of organisms is fairly balanced by the difficulty of conceiving how the phenomena of organic nature can be accounted for without it. The point which we have laboured over is that, if science has no call to settle the question, it has none to prejudge it. It was only because Dr. Romanes seemed to me unwittingly to have done so, that I ventured the criticisms which opened this discussion.

Cambridge, Mass., U.S. ASA GRAY

P.S.—A brief note upon Mr. Hannay's letter, NATURE, vol. xxvii. p. 364, referring to my supposition of successive generations slowly changing, "*yet always so as to be in compatible relations to the environment.*" He remarks, this "is just such a statement as 'Design' would require, but cannot be held by scientific evolutionists, otherwise why are there so many extinct species?" Surely it could be held by the soundest of evolutionists, for it is of the very essence of Darwinism. Are not the individuals which compose the present fauna and flora in compatible relations to the environment, and is not the extinction of species going on? In human society do we consider that the unmarried and the childless members of the community are not in compatible relations to their surroundings? Is there any reason to suppose that the individuals of a flora of earlier times—say of the Miocene—were not on the whole in as orderly and compatible relations as the existing flora is? It is not *chaos* but *cosmos* that true Darwinism has in mind, common though the contrary impression be. A. G.

PROF. ASA GRAY is kind enough to remark that he has read my reply to his previous communication with interest. I should like to say, *in limine*, that I have read his reply to me not only with interest but with profit; for it is not often that one meets with an argument so carefully thought out and so clearly presented. Therefore, if I seek to meet his further criticisms, it is not in any spirit of controversy that I do so, but solely for the sake of endeavouring to help, so far as I am able, in determining the true logical position of an important question.

This question, as Prof. Gray observes, is a narrow one, and I shall keep to it. Without therefore trespassing upon the wider question of Theism as a whole, our discussion is confined to "an inquiry whether certain inferences may or may not *scientifically* be drawn from certain premisses."

First, I have to meet the dilemma which is put to me when I am told that, having said there is no point of logical contact between natural science and natural theology, I ought not forthwith to say that natural science is competent to destroy an inference belonging to natural theology. But in stating it as my opinion that natural science had shown the inference previously drawn to be invalid, I did not myself, as my critic asserts, draw any inference (even of a negative kind) from natural science to natural theology; I merely endeavoured to point out that an inference previously drawn from the one to the other was illegitimate, that inasmuch as the inference proceeded from natural science it was liable at any time to be overturned by natural science, and that it had now actually been overturned. Whether or not, therefore, I was right in saying that there is no point of logical contact between natural science and natural theology, at least I did not myself endeavour to institute such contact.

But I am told, you admit that long ago the inference in question was valid, and even cogent. Well, I answer in one sense it was, but in another and a truer sense it was not. For its cogency arose from the hypothesis of special and sudden creation on which it rested; grant this hypothesis, and the inference from organic adaptation to intelligent design becomes not only cogent but inevitable. The hypothesis, however, was not one that really belonged to natural science, and it was just this hypothesis that constituted the "fictitious logical connection" alluded to in the passage which Prof. Gray quotes from my previous letter. The facts presented by science remain, of course, very much the same as they were; but it does not follow that, in the absence of the special creation hypothesis, "whatever evidences of intellectual origination these manifested were *seen in the things themselves*, and we suppose are to be seen there still." Let us take an illustration. In the last issue of NATURE there is a letter from Prof. Darwin describing the formation of mudballs by a suitable and rare combination of natural causes. He and his brother did not see these balls in process of formation, and therefore he says, "On seeing the first one or two, they looked to us like the handiwork of some boy with an enthusiasm for mud pies"; but their number and the constancy of their situation on the slopes of hills—*i.e.* further knowledge of the inferred conditions of their origin—afterwards disposed of the teleological hypothesis in favour of a physical one. Now here it is equally true that "whatever evidences of intellectual

origination these manifested were *seen in the things themselves*," and after the hypothesis of their physical origin had been arrived at, were "to be seen there still." Yet we should have deemed the brothers Darwin very unworthy representatives of their family if, after having arrived at the physical hypothesis, they had continued to argue in favour of a teleological enthusiasm for mud pies, on the ground that "the inference was not one from an intelligent originator to design in the (in-)organic world, but from marks . . . in the latter which indicated design to an intelligent originator." In other words, a change in the hypothesis concerning the *origination* of the mudballs entirely changed the logical cogency of the teleological inference.

Now I have purposely chosen this illustration because it is of so simple a character, and therefore serves in a clear manner to show how greatly a teleological inference may be modified by a change of hypothesis concerning the mode of origin of a structure, even though the structure remains the same; if there had been no evidence of a purely physical mode of origin in this case, it might truly have been said of the teleological interpretation, "the inference to most minds was convincing; at least it was legitimate." Of course in organic nature the apparent marks of design "in the things themselves" are much more numerous, varied, and complex than any that we meet with in inorganic nature; but no matter how numerous, varied, and complex such marks of design may be, if we see good reason to conclude that they have *all been produced by physical causes*, they are no more available as *evidences* of special design than are the mudballs—although both they and the mudballs, being alike formed under an orderly system of causation, may be due to a general design pervading the cosmos. And here I understand that Prof. Gray is in agreement with me, for he says that when I assign the whole results to known [or unknown] physical causes and discard the factor of intelligence, I am bound to render their adequacy at least conceivable. This appears to show that Prof. Gray is at one with me in holding that physical causes as such do not constitute other or better evidence of design in the organic than in the inorganic world; and it is only because he cannot conceive how such causes are adequate to produce the results observed in the former that he deems these results unique as evidence of "the factor of intelligence." In other words, supposing for the sake of argument that all these results have been due to purely physical causes, and supposing further that all these causes were as perfectly well known as the less complicated physical causes of the inorganic world, then I take it Prof. Gray would agree with me in saying that under such circumstances the former would constitute no other or better evidence of design than the latter.

If so, our only difference resolves itself into a difference in the estimate which we respectively form of the probable adequacy of purely physical causes to produce all the results which are observable in organic nature. To me the probability appears overwhelming that in respect of method "all nature is of a piece," and therefore that the terms "physical" and "natural," when applied to causation, are logically, as well as etymologically, convertible. To Prof. Gray, on the other hand, the probability appears to be that such is not the case, but that, when we meet with the "*direction of action to ends*," we have special evidence of "the factor of intelligence," which therefore makes nature "of at least two pieces," and so makes the term "natural" to mean more than the term "physical."

Supposing that I am right in understanding this as the only difference between us, I may point out that if, while following my ideas of probability, I have erred on the side of rashness in drawing "the downright conclusion" that the facts of organic nature present no other or better evidence of design than the facts of inorganic, Prof. Gray, in following his ideas of probability, can scarcely be able to shut out the suspicion (more especially in view of abundant historical analogies) that, in resorting to "the factor of intelligence" as a hypothesis wherever physical causation is found to be complex or obscure, he may be merely supplementing our present ignorance of such causation by an inference which is at least as rash as my statement.[1] And here I should like to observe, with special reference to the natural or physical causes summed up in the term "natural selection," that although I speak with all the respect which I sincerely feel for so distinguished a naturalist and so able a dialectician, I am not able to follow Prof. Gray in his understanding of this subject. For he says of the theory of natural selection that it is destitute of any pretensions to act as the substitute of the theory of special design, "until it is explained how the physical destruction of a part should have set the rest into varying at all, into varying advantageously, and into varying into the very special ways they have done." But surely it is no part of the theory of natural selection to suppose that the *physical destruction* of unfit organisms is, or has any need to be, the *cause* of advantageous variations arising in other and allied organisms. The theory merely supposes that variations *of all kinds and in all directions* are constantly taking place, and that natural selection seizes upon the more advantageous. Therefore, so far as this theory is concerned, there is no call to explain why promiscuous variation occurs; it is simply a fact that it does occur, though not necessarily *made to occur* by the destruction of other organisms. Neither is there any call to explain why the variations occur in special and advantageous ways, for they are not supposed to occur in special and advantageous ways, but only to appear to do so on account of all other variations being eliminated, while those which happen to occur in the specially advantageous ways are preserved. Again, Prof. Gray says in his postscript that the theory of natural selection supposes successive generations to be slowly changing, "yet always so as to be in compatible relations to the environment." Now it is true that where the changes in the environment are gradual, and the variations of specific type are being slowly accommodated to them, each generation is, on the whole, in compatible relations with its environment. But it is not true that such continuous compatibility in itself points to design; it only points to the plasticity of the varying type, which, if not sufficiently plastic to meet the new demands upon it in this respect, simply becomes extinct.

In conclusion, I agree that "natural science leaves aside the question whether evolution and design in nature are compatible or not," and I agree that, "if science has no call to settle the question, it has none to prejudge it." But I do not agree that I have prejudged this question by saying that in my opinion the theory of evolution, in supplanting the theory of special creation, has necessarily removed the special evidence of design in organic nature, by showing that in respect of causation organic nature and inorganic nature are one. GEORGE J. ROMANES

[1] I suppose it will be admitted that the validity of an inference depends upon the number, the importance, and the definiteness of the things or ratios known, as compared with the number, importance, and definiteness of the things or ratios unknown, but inferred. If so, we should be logically cautious in drawing inferences from the natural to the supernatural; for although we have the entire sphere of experience from which to draw an inference, we are unable to gauge the probability of the inference when drawn—the unknown ratios being confessedly of unknown number, importance, and degree of indefiniteness; the whole orbit of human knowledge is insufficient to obtain a parallax whereby to institute the required measurement or proportion between the terms known and the terms unknown. Or, otherwise phrased, we may say—As our knowledge of a part is to our knowledge of a whole, so is our inference from that part to the reality of that whole. Who, therefore, can say, even upon the supposition of Theism, that our inferences or "idea of design" would have any meaning if applied to the "All-Upholder," whose thoughts are not as our thoughts?

The High Springs of 1883

THE high springs of the present year, consequent upon the excessive rainfall of the past winter, are an event that ought not to pass unrecorded in the pages of NATURE. I can speak only of phenomena which I have observed upon my native chalk hills of Hampshire, but I doubt not that similar facts have attracted attention elsewhere.

The Candover, a confluent of the Itchen from the north, burst forth this year in a field near Preston Candover, where it has not been known to rise for the last fifty years, and has flooded the road between Preston Candover and Chilton Candover. The Itchen itself rose in the valley above Cheriton beyond its recognised source, and has flooded fields on the road to Kilmeston, where no one recollects to have seen water before.

The Hampshire tributaries of the Thames have acted in exactly the same manner. The Whitewater has issued forth in the valley just below Upton Grey, far above its usual origin even in the highest springs, and has flooded the whole road between Bidden and Greywell. Another branch of the same stream has risen in the fields on the left of the main road from Odiham to South Warnborough, where spring water has never been known within the memory of the oldest inhabitant. In like manner the Wey, which, in wet seasons, takes its rise in the meadows adjoining Chawton House, has issued forth this year at a much higher level in the fields below Farringdon.

These facts are the more worthy of notice because it has been generally believed that, in the Hampshire hills at least, owing to more efficient drainage and other causes, the springs were